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AUTHOR Peterson, Marvin W.
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ABSTRACT

Purposes of research in higher education, the issue of research paradigms, and the identification of research questions are discussed in this keynote panel paper. Different purposes of research include conceptual and theoretical development, relating theory and practice, and improving practice. Higher education as a field has not yet given much attention explicitly to the role of research in conceptual or theoretical development. Research can help in translating concepts and theory and showing their fit with practice. Most of the published articles in higher education currently place research in some theoretical or conceptual context. If research is directed to improve practice, the focus is real issues and the effort is likely to be organized around functional categories, such as teaching and learning and financial affairs. Different research purposes suggest various research strategies and methods. In considering topics for research, two criteria are important: significance and interest. Seven suggestions for higher education research are offered, including: identifying significant conceptual, issue, and functionally-based research topics; promoting integrative research efforts that combine varied strategies and methods; and initiating more professional discussions and publication activities to examine the applicability of new approaches. (SW)

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# CRITICAL CHOICES:

# FROM ADOLESCENCE TO MATURITY IN HIGHER EDUCATION RESEARCH

by
Marvin W. Peterson, Director
Center for the Study of Higher and Postsecondary Education
University of Michigan

Prepared for
KEYNOTE PANEL:

"Of Trees and Fruits and the Future:
Whither Higher Education Research?"

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ASHE Association for the Study of Higher Education
The George Washington University/One Dupont Circle, Suite 630/Washington, D.C. 20036
(202) 296-2597

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## INTRODUCTION

Having read George's Change article "Trees Without Fruit" (Keller, 1985) in which he chided us about the state of research in higher education, I am pleased to see that he has at least classified us as at the "adolescent" stage since that is the image I chose to describe organizational theory and research in an article in the Educational Research last year. Our difference is perhaps one of tone -- he describes adolescence as a "time of confusion, clumsiness, and semi-formed notions". I described it as a time of struggle with problems of "identity and commitment, relevance, and legitimacy". You see I am already part of his caricature of how we are caught up in the "social science" paradigm and jargon.

Actually in labeling something as adolescent, I feel somewhat as I do about quality or excellence. I am probably not an expert and cannot specify my criteria for adolescence but, having just shepherded one child through that stage and having another who just entered it, I recognize the stage when I see it. I also know that when one observes the phenomenon long enough, it affects you as well. I finally sent my older daughter from Michigan to California to Jack Schuster and Howard Bowen's institution to complete her maturation. Now having seen the bills, I know something about the economics of the enterprise and the economic condition of the professoriate that I did not learn from their book. So as we examine the adolescent stage of research in higher education in this panel and the discussions, I expect our perspectives will be also altered.

I agree with George that our research has had limited impact on practice although I think his expectations may have been unrealistic given the checkered pattern of social science research in general. I also concur with

his assessment that we are dominated by a social science research paradigm although I think it is changing and has not been quite as debilitating as he suggests. Finally, we are in complete agreement about the excitement and challenge facing research on higher education at this time.

My role today is probably that of the "rational analyst" (some would say "apologist") between my predecessor, the "constructive critic", and Yvonna, our "chiding conscience". I think I am the "ego" between the "id" and the "libido", but you will have to decide which of them is which. George's perspective is broad and sweeping, connecting our higher education research within the larger realm of higher education and educational research. Mine is focused more on our academic and research community in higher education. Yvonna, I believe, will be challenging us to focus on the current context of higher education; to change our views or assumptions about it.

My intent is to suggest three critical choices or debates our emerging field might have. The first relates to George's issue about the purpose of higher education research; the second, elaborates on the need for a debate on research paradigms; and third, examines how we identify important research questions. I want to do this against the backdrop of the emerging research in higher education and to suggest some concerns as we strive to move it ahead.

## PURPOSES: TRICHOTOMY, PRIORITY OR UNITY

All of us involved in the academic study of higher education are interested in understanding its context, its institutions, its processes, and its participants or constituents, but as researchers and as scholars we approach it with slightly different primary purposes or motives: concern for

conceptual and theoretical development, or for relating theory and practice, or for improving practice. While these purposes overlap, I want to discuss them separately since they suggest somewhat different implications for the role of the researcher, definitions of our primary constituency, views of the structure of higher education research, and emphasis on research strategy or methods.

Theory Development

The development of new concepts or theories suggests the primary role of researchers is that of a "conceptualizer" of our field and its phenomenon. Correctly or incorrectly we think of "academics in other disciplines" and their associations and research journals as the primary constituents to whom we turn for ideas, research methods and respectable outlets for research. (George would argue we turn too heavily to the social science disciplines). In this purpose the classification of higher education research takes on a "cross disciplinary" mode: the history, philosophy, economics, politics, sociology, psychology, organizational and administrative behavior, etc. of higher education. Interestingly the implications for research methods are "not distinct" - advocates of qualitative methodology stress its usefulness in obtaining new conceptual insights and in understanding the theory; advocates of quantitative methodology point to the index construction, clustering and relational techniques that can assist in analyzing and observing patterns to provide conceptual insight and to examine theoretical explanations.

As the field of higher education research has emerged, I would suggest that there is little research to point to that has contributed to original conceptual or theory development. Some exceptions as examples from the

organizational area might include the notions of organizational saga (197 ) and anarchy (19 ) - but even those are partially adaptations from other areas. The major theoretical contributions would appear to be expanding the generalizability of theory borrowed from other settings to higher education (e.g., political view), showing the limits to concepts or theories that do not fit very well (e.g., bureaucracy) or modifying them to fit higher education (in the case of both). The major contributors have been either researchers from other disciplines who occasionally focus their research on higher education or those who have migrated from other fields to higher education. A disturbing trend is that as higher education matures more faculty are trained in higher education programs (albeit some with good disciplinary backgrounds) and, it is my impression, fewer higher education faculty remain active in disciplinary associations and vice versa for disciplinary faculty in groups like ASHE.

While our research does not yet reflect a "cross disciplinary" structural taxonomy or set of categories, our higher education courses often reflect it as do some of the ASHE clustering of papers. This may enhance importing conceptual thinking from other fields but the issue is to what degree it is useful.

Higher education as a field has not as yet given much attention explicitly to the role of research in conceptual or theoretical development. Since much of conceptual and theoretical development involves thinking about the meaning of research, it may be important to encourage synthesizing and mapping conceptual ideas against our research. Bob Silverman's mapping of some higher education journal manuscripts in the Review of Higher Education (1982) notes the paucity of articles in his "conceptual theory" category. Articles

synthesizing higher education research in journals like the Review of Educational Research that stress conceptual or theoretical reviews are still sparce. AERA - Division J's new Annual Handbook of Theory and Research in Higher Education (1985) may help fill this void. Sessions at ASHE or AERA - Division J devoted exclusively to issues of theory development or the role of research in this purpose are almost non-existent.

This impressionistic picture of higher education research in theory development is somewhat ironic. In a field in which we often argue the uniqueness of our institutions, processes and participants, we still give little attention to trying to develop a unique, research-based theory. For example -- despite the fact higher education institutions focus on learning as their primary function, we still do not have a cohesive organizational theory that reflects that function.

Theory and Practice

If the purpose of research is viewed as relating theory and practice (note I said and practice not to practice), the primary role of the researcher becomes one of "translator" who can aid in sifting concepts and theory and showing their fit with our higher education phenomenon. Here our primary colleagues and constituents are often those engaged in the profession. Such a purpose suggests a view of the field and its research that is more "interdisciplinary" - clustered around important broad issues to be studied that draw on several disciplines and broadly affect our institutions: innovation, institutional decline, racial integration, interinstitutional coordination. Research methods and strategies are again eclectic. But important issues generally suggest the need for large scale studies which use

comparative or longitudinal approaches or coordination and synthesis of smaller studies. We are interested in whether theories or concepts fit, which among alternatives are more useful, or in finding new combinations which explain the behavior under examination. Diverse methods -- both quantitative and qualitative -- are useful.

Here higher education research probably has advanced further. Most of our published articles now place the research in some theoretical or conceptual context. We can all identify several conceptual or theoretical perspectives that have been used to study phenomenon in our areas of interest.

Clearly the primary contributors here are from our "higher education academic programs and research groups" and they have become each other's constituents as well. ASHE and AERA Division J offer the primary opportunities to present and discuss our research which have titles typically describing the concepts, method, and problem focus -- e.g., on "a comparative, ecological analysis of the role of peer cultures in retention" or "an ethnographic examination of faculty anomie and early retirement in a small liberal arts college." Our journals -- Journal of Higher Education, Review of Higher Education, and Research in Higher Education all provide an outlet to communicate with each other. We are less likely to publish either in the academic journals or in the practitioner journals. We may be becoming translators of theory and practice primarily among ourselves and to our graduate students. However, I think we have contributed greatly, if indirectly and in a cumulative fashion. For example, administrators are now comfortable with our varied conceptions of organization theory and decision making and often even recognize that their institutional dynamics may be

simultaneously bureaucratic, anarchic, loosely coupled, political, and consensual and can recognize and deal with that complexity.

The interdisciplinary and issue-oriented structure of our research suggested by this theory-practice purpose is often reflected by topics in the ASHE ERIC Research Report Series. Clearly topics like retention, effectiveness, decline have benefited from synthesis vehicles for publication.

Since theory-practice issues are extensive and large scale studies are seldom funded, the problem in enhancing the theory-practice research purpose may also be one of finding effective ways for researchers in different settings to work together collaboratively on research or synthesis. Opportunities like the NIE panel on Involvement In Learning are important but are not dependable or frequent vehicles.

Improving Practice

The third research purpose, concern for improving practice, suggests the primary role of the researcher is that of "action researcher" - dealing with real issues and involving practitioners in research and improvement efforts. The primary constituents are the practioners - students, faculty and administrators - both for defining what to study and as the focus of our dissemination and communication. Higher education research in this purpose is more likely to be organized around "functional" categories -- leadership, governance, management, teaching and learning, financial affairs, student affairs, etc. - or "specific or immediate problems" -- budget reduction, faculty reallocation, etc. Research methods and strategy suggest greater emphasis on things like policy research, evaluation research, and action

research approaches stressing participant involvement as well as a heavy emphasis on utilization and immediate dissemination strategies.

It is in this purpose that I would suggest there is both a considerable amount of higher education research and considerable criticism and frustration. There are several reasons.

First, important practical policy issues are not easily researched and the policy process does not always rely heavily on research. State-level attempts to improve quality is an example. More specific institutional problems are often ephemeral or demand immediate solution -- leaving researchers to provide quick summaries of what is known, descriptive profiles of what seems to work, or applied assessments of policy alternatives.

Second, as higher education researchers we give little attention to action research strategies that involve participants in identifying the research agenda, designing and participating in the research, and in its eventual implementation and evaluation in order to improve the usefulness, credibility and acceptance of the findings. This is ironic given the fact that most of our graduate students will enter administrative or analytic positions (not academic careers) and could benefit from such training.

Third, I detect a growing separation between higher education researchers and our administrative constituents. Although I have no firm data, I see fewer higher education researchers active in administrative associations or reporting in their publications. The exceptions are, of course, notable -- the researcher whose timely and comprehensive knowledge of current problems makes him or her a highly visible spokesperson. The reasons for this separation are probably several: decreased faculty travel budgets, the political distancing of administrators from faculty during budget reductions,

the growth in numbers of institutional researchers and administrative policy analysts who carry on this role, the increasing number of higher education faculty who were trained as such and have limited administrative experience, and our own attempts to increase the sophistication of our research as the field has emerged.

Finally, a positive. Numerous publication series provide an outlet for research syntheses organized around functional areas on current problems. The Jossey-Bass New Directions Series, many of the ASHE-ERIC Research Report Series, and the many association monographs come to mind. The drawback is they vary in quality, often are more literature than research based, and do not provide a comprehensive functional or problem mapping of the field.

This overview suggests that different broad research purposes for our field stress different roles, constituents, structures and strategies for our research and may be differentially developed. The overriding issue it raises is this:

1. Should the higher education research profession stress one purpose over the others, seek to give them a more balanced emphasis, or seek a unifying view of them?

## PARADIGM AND METHODOLOGY: THE NEED FOR DEBATE

Turning briefly to my second topic, it is apparent that the different research purposes suggest a wide variety of research strategies and methods. It is also clear that different research paradigms may be useful regardless of the purpose being pursued.

In a review of research articles in our higher education journals the past two years, one can find virtually all strategies (except experimental), varied designs and many methods and techniques -- both quantitative and qualitative. Increasingly one finds more complex research studies or programs that combine several strategies, designs and different methods. What is disturbing is that in reviewing the higher education journals, there are very few articles which discuss the application of specific research strategies or methods in higher education such as Cliff Conrad's discussion of "Grounded Theory" or Zee Gamson and Terry Rogers' of "Evaluation as a Developmental Process." Again given our contentions about the uniqueness of the nature of higher education and the importance of modifying research strategies and methods to study it, there may be a need for timely discussions of them as well as critical assessments of how useful they are. One contributing factor to this condition may be the rather limited attention our graduate programs give to statistics and research design. Having participated in a variety of higher education doctoral program reviews, it is my experience that requirements in this area are often minimal and fulfilled by an array of elective courses elsewhere in the host School of Education and university.

In a broader sense, the debate over research paradigms has gone on in other fields for over a decade. The terminology varies: traditional, conservative, social fact or quantitative paradigm vs. the cultural, radical, social definition or qualitative. This debate has scarcely been touched in the field of higher education. An occasional article such as Ernie Pascarella's "Perspectives on Quantitative Analysis" (1982) or Ian Mitroff's "Secure Versus Insecure Forms of Knowing" (1982) and occasional panels on quantitative or qualitative methods at our professional meetings have barely

introduced the topic let alone the debate. In fact we are so far behind an article by John Smith (1986) in a recent Educational Researcher suggests the debate has been largely closed and needs to be reopened.

The point of this digression suggests the second critical choice:

2. Do we as an emerging profession need to give more serious attention to examining research methods and their applicability in higher education and to a serious discussion of various research paradigms for our research?

## SIGNIFICANT AND INTERESTING

My final topic touches on how we identify important topics for research. Let me suggest two criteria: significance and interesting.

By significant I am referring to broad topics -- be they conceptual, issue oriented, or functionally based -- in which there is a paucity of good research and where there is a sense that timing, interest and resources might support a probable, substantial contribution. For example, some significant areas of research might include: conceptual thinking about the nature of learning organizations; interdisciplinary theory-to-practice issues such as the impact of information and telecommunication technology or institutional responsiveness to minorities; or practice oriented, functional topics like linking resources to quality improvement.

Criticisms of the development and impact of higher education research are in part a product of the lack of funding for major long-term research efforts. They also reflect different views of important topics, e.g., a recent study by Cameron (1985) which surveyed college and university

administrators regarding significant issues needing research differed substantially from a content analysis of recent research reported in our higher education journals. But we also lack a coherent, coordinated pattern of research that occurs in other fields. The efforts at research synthesis, previously noted, are usually narrowly focused on a single topic although AERA Division J's sponsorship of the Annual Yearbook on Theory and Research - Higher Education (1985) may be an exception. How such an effort to map significant conceptual, issue or functional gaps might be accomplished, and how it should be initiated is unclear. However, such an effort could focus significant areas for research attention and provide a vehicle for collaboration among our dispersed colleagues -- something the current NIE (OERI) Research and Development Center competition has done.

By interesting research I am suggesting a different criteria identified by Murray Davis (1971) in a provocative article entitled "That's Interesting" in which he noted that theories were interesting not because they were correct but because they challenged commonly held beliefs, assumptions and propositions. Identifying and encouraging such research could assure an intellectually vital field and provide alternative concepts or models to be tested against existing ones -- supporting what John Platt called "Strong Inference" research -- research that pits alternative explanations which can move a field ahead more rapidly. Again how this might be accomplished is unclear -- but it could certainly enliven dull research presentations.

The third critical choice I am suggesting is thus:

3. Can and should steps be taken to map, identify and promote significant conceptual, issue, and functionally based topic areas of research and can interesting different perspectives be encouraged?

## SUMMARY

It is not possible to leave you without stating my own preferences for moving from adolescence to maturity. Seven "i"s may assist.

Regarding purposes, I would stress a "balanced" emphasis by:

1. Guarding against our growing <u>insulation</u> which cuts us off from the disciplines to insure the influx of conceptual ideas and methodological debates.
2. Guarding against <u>isolation</u> from administrators and practitioners to assure their practical insights and interest.
3. Guarding against <u>isomorphism</u>, freezing our research categories or discussions only along conceptual or theory-practice issue or more practical functional lines.

Regarding research methods and paradigms, I would emphasize:

4. <u>Initiation</u> of more professional discussions and publication activity aimed at examining the applicability of new methods and strategies and to understand new or different paradigms.
5. Promoting <u>integrative</u> research efforts that combine varied strategies and methods and encourage researcher collaboration. Significant and interesting topics will require it.

Regarding significant and interesting topics, we should engage in:

6. <u>Identification</u> activities which map our field and its research to focus significant topics.

and

7. <u>Illumination</u> activities to look for interesting new perspectives to challenge us.

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